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Research Note 86-67

AD-A169 261

CONTROLLING THE TEMPORAL STRUCTURE OF LIMB MOVEMENTS:  
A RESPONSE

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Research Institute for the Behavioral and Social Sciences

May 1986

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Unclassified

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

| REPORT DOCUMENTATION PAGE   |                       | READ INSTRUCTIONS<br>BEFORE COMPLETING FORM                                |
|---|-----------------------|--|
| 1. REPORT NUMBER<br>ARI Research Note 86-67   | 2. GOVT ACCESSION NO. | 3. RECIPIENT'S CATALOG NUMBER  |
| 4. TITLE (and Subtitle)<br>Controlling the Temporal Structure of Limb Movements: A Response   |                       | 5. TYPE OF REPORT & PERIOD COVERED<br>Interim June, 1985                   |
|   |                       | 6. PERFORMING ORG. REPORT NUMBER   |
| 7. AUTHOR(s)<br>Richard A. Schmidt  |                       | 8. CONTRACT OR GRANT NUMBER(s)<br>MDA 903-85-K-0225                        |
| 9. PERFORMING ORGANIZATION NAME AND ADDRESS<br>Motor Control Laboratory<br>Department of Kinesiology<br>University of California, Los Angeles   |                       | 10. PROGRAM ELEMENT PROJECT, TASK AREA & WORK UNIT NUMBERS<br>2Q161102B74F |
| 11. CONTROLLING OFFICE NAME AND ADDRESS<br>U.S. Army Research Institute for the Behavioral and Social Sciences<br>5001 Eisenhower Ave., Alexandria, VA 22333  |                       | 12. REPORT DATE<br>May 1986  |
| 14. MONITORING AGENCY NAME & ADDRESS (if different from Controlling Office)   |                       | 13. NUMBER OF PAGES<br>9   |
|   |                       | 15. SECURITY CLASS. (of this report)<br>Unclassified                       |
|   |                       | 15a. DECLASSIFICATION DOWNGRADING SCHEDULE                                 |
| 16. DISTRIBUTION STATEMENT (of this Report)<br><br>Approved for public release; distribution unlimited.   |                       |  |
| 17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)  |                       |  |
| 18. SUPPLEMENTARY NOTES<br><br>COR: J. Orasanu  |                       |  |
| 19. KEY WORDS (Continue on reverse side if necessary and identify by block number)<br>mass-spring control<br>motor-programs.<br>kinematics<br>limb movement<br>EMG.   |                       |  |
| 20. ABSTRACT (Continue on reverse side if necessary and identify by block number)<br><br>This report is a response to a recent article by Berkinblit, et.al., which suggested that motor-program models, in which a centrally programmed temporal structure was a key ingredient, seemed incorrect. In particular, Schmidt takes issue with their discussion of Fel'dman's (1974) mass-spring model which denies a central program for limb movement timing and amplitudes. Schmidt argues that evidence for unidirectional actions and sequential movements limits the utility of the mass-spring model. |                       |  |

The Berkinblit et al. (1985) target article presents so many new and exciting ideas about movement control that I feel frustrated in not having more space here to agree, amplify, or criticize. But I found particularly fascinating the discussion of "mass-spring control," and here I direct my remarks at the role of centrally programmed timing in such models.

An important event was Polit and Bizzi's (1978) finding that deafferented monkeys could move reasonably accurately to unseen targets, even when perturbed briefly. Such findings, when put together with a number of others (e.g., Kelso & Holt, 1980; Schmidt & McGown, 1980), suggested that motor-program models, in which a centrally programmed temporal structure was a key ingredient, seemed incorrect (Schmidt, 1980, 1982). These findings supported theories in which only an equilibrium point was specified, with the limb moving to the endpoint as a mechanical system. Turvey's (1977) popularization of Fel'dman's (1966, 1974) original model encouraged a "dynamical" viewpoint (e.g., Kelso, Holt, Kugler, & Turvey, 1980; Kelso, Holt, Rubin, & Kugler, 1981), stressing that the central control of timing was not necessary. Berkinblit et al. seem to take this perspective also, but they convey a mixed message, as I will try to point out here.

Other perspectives are possible, but I view the mass-spring models<sup>1</sup> as being important because they eliminated the need for central timing in movement. They were elegantly simple, such that specifying a minimal number of parameters

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caused the temporal structure of the action (i.e., the kinematics) to emerge as a consequence of the dynamics involved. One apparent problem (Wallace, 1981) was their seeming inability to account for the phasic EMG activities seen in rapid limb movements. But in the target article, the authors review Fel'dman's (1974) arguments about how even the temporal EMG structure can emerge from the dynamics (see Figure 4F). Indeed, the authors conclude that "It is unnecessary to suggest that there is a 'central program' for (the EMG bursts') timing and amplitudes..." (p. 19). There are two major reasons why I feel that this conclusion is not warranted, at least in general terms.

Unidirectional actions. First, there is evidence that contradicts the Fel'dman (1974) model, and the  $\lambda$  model presented in the target article. Wadman, Denier van der Gon, Geuze, and McI (1979) and Shapiro and Walter (1985) have used quick (150-ms) positioning movements, <sup>which</sup> typically show a phasic EMG pattern: the agonist is activated, the agonist is abruptly terminated as an antagonist burst occurs, and then the agonist burst appears again at the end of the movement, presumably for stabilization. An important finding comes from a paradigm in which the limb is occasionally unexpectedly blocked mechanically, so that no movement can occur on that trial. On such trials, the temporal structuring of the EMG activities is remarkably similar to that seen in the "normal" unblocked trials, especially during the first 100-120 ms, with some

(probably) reflex-based modifications later in the pattern. The question is, What (or who?) is responsible for the temporal structure seen in the "normal" and blocked trials? To me, the structure cannot have simply emerged from the dynamics as Fel'dman (1974) would have it, as the dynamics were massively disrupted by the mechanical block; yet the EMG timing was nearly unaffected. A better hypothesis, in my view, is that this temporal structure is centrally organized, and "run off" as the movement unfolds. I take these findings as evidence against Fel'dman's (1974) view in particular, and the dynamical viewpoint (Kelso et al., 1980, 1981) in general.

I am confused slightly by the authors' position about the temporal structure of single positioning actions. After they argue that such responses are not timed centrally, they say that "movement speed may depend on the speed of shift of the equilibrium point..." (p. 17; see also p. 38). Now, if we have the equilibrium point shifting from one value to another at different speeds, is this not tantamount to an underlying temporal structure for the action? I see this solution -- together with that of Bizzi, Accornero, Chapple, and Hogan (1982), that the equilibrium point "sweeps" continuously (presumably under central control) -- as a necessary, but disappointing, retreat from the elegant mass-spring view that had no temporal structure at all. And, in support of the sweeping equilibrium point, Berkinblit et al. cite a model (Adamovich & Fel'dman, 1984) and supporting data (Adamovich, Burlachkova, & Fel'dman, 1984) in which a centrally regulated

temporal structure is a critical, dominant feature. In fact, there are as many as three separate timing structures in this model, and more if more movements are "superimposed," as they term it.

Sequential movements. Second, the mass-spring model does not hold very well for sequential movements. Using rapid reversal movements (Schmidt, McGown, Quinn, & Hawkins, 1985), the subject moves a lever to a target, reverses direction, and follows through beyond the starting position, all with a goal movement time of approximately 300 ms. Unexpectedly loading the limb inertially causes the reversal point to be shorter than on normally unloaded trials, which contradicts the notion (Kelso et al., 1981) that the limb is behaving as an oscillating mass-spring system. This result can be understood in terms of an underlying, relatively unchangeable, temporal structure, where a correction for the added load could not be made before the previously timed antagonist burst "arrived" to reverse the limb's direction. These results also bring into question the generality of the equifinality notion (see also Schmidt, 1980; Schmidt & McGown, 1980; Schmidt et al., 1985).

But Berkinblit et al. have extended the mass-spring modeling to long movement sequences in a clever way, suggesting that the limbs achieve a series of equilibrium points which together make up a sequential action. But in doing so, the role of central timing in movement production now becomes a dominant part of their model. They note (p. 39) that the

temporal structure of the frog's actions appears to be sped-up and slowed-down as a unit, which suggests "the existence of a common sweep generator (whose rate of sweep) is a controllable parameter." As far as I can tell, this is just the idea that has been used in the concept of a generalized motor program (Schmidt, 1975, 1982) which they have denied earlier. The distinction seems to be between (a) the timing of the sequence of shifts in equilibrium point, which is controlled centrally, and (b) the timing involved in the achievement of each of them, which they argue is not timed centrally, and with which I disagree.

Since the existence and role of central timed structures are perhaps the most critical issue distinguishing motor program theories (e.g., Schmidt, 1982) from dynamical theories (Kelso et al., 1980, 1981) for movement control, I am disappointed that the authors are ambiguous about the role of timing in their model. The evidence and arguments here point me strongly toward the necessary existence of such structures in theories of movement control.

#### NOTES

1. The authors correctly point out that there are two (or perhaps even more) mass-spring models -- the  $\alpha$  model from Polit & Bizzi (1978), and the  $\lambda$  model discussed here.



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